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March 2, 1981

Dr. French Anderson
Building 10, Room 7D18
National Institutes of Health
Bethesda, Maryland 20205

Dear Dr. Anderson:

I am sorry that I am a couple of days behind in answering your request, but a close relative learned that he had a serious medical problem at the same time. Trying to help with him threw me off schedule.

Enclosed are my available reprints on this subject. There is an additional two talks in Japan at a symposium that I do not have a copy of and two papers written with the European groups that I have no more copies of. I think the letter to Science enclosed answers most of your questions.

Before any treatment with the virus was undertaken the Groups from Colgne, Antwerp, Bern, and myself discussed what we planned to do with the parents at length in as great a detail as we knew and obtained a standard pre-operative permission. I do not know precisely how the information was released but the patients were followed by most of the newspapers in the world and by BBC so what was being done was clearly not secret. As a matter of fact, the best continuing information I had on the condition of the patients was received through ~~Harold~~ HORACE Judson, a free-lance Life-Time correspondent who was based in nearby Paris at that time.

Only the oldest child received two doses of purified virus in the amount that we gave rabbits per Kg. It is of interest that it is only this child which seems clinically better though we do not have more objective biochemical tests as the parents did not want these tests repeated. Whether the youngest child received live virus at all is a matter of question, as the extremely purified virus is unstable and did not prove active upon testing when a sample was returned to me after injection.

In a very general sense one thing that is often over-looked is the fact that live viruses were used in the treatment of cancer in the 1950's by Alice Moore and Rhoads at Sloan-Kettering. Also, live cancer cells were implanted in prisoners in Ohio at about this time.

As the people who did this were knowledgeable and could have predicted that the transplantation would not be successful, I could only think that the possible presence of a virus was being tested.

The papers that I do not have reprints of are:

Rogers, S., General Implications of Gene Therapy, Japan Industrial Technology Association, Symposium on Future of Life Sciences, Tokyo, 1972.

Rogers, S., Experimental Background of Gene Therapy, JITA as above.

Terhegen, H.C., Lowenthal, A.M., Columbo, J.P. and Rogers, S., Unsuccessful Trial of Gene Replacement in Arginase Deficiency, Z. Kinderheilk, 119:1, 1975.

Columbo, J.P., Terhegen, H.C., Lowenthal, A.M., Van Sande, M. and Rogers, S., Inborn Errors of Metabolism Hormones, Argininemia and Van Den Berg, (eds) Academic Press, London, 1973.

Aside from what is above, I have also been concerned with the reason for the "moratorium" on this research area. I stopped, but those that had funds private or otherwise did not, and they were the ones instigating the moratorium. The broad patent obtained recently by Stanford and Berkeley is a case in point. In a telephone conversation with Boyer, for example, when I was trying to get a small amount of his restriction enzyme (used by Jackson and Berg) he wished to know why I wanted it. When I told him it was to make efforts to add nitrogen information to the cauliflower mosaic virus he said that he had not thought of that. Needless to say, I did not get any enzyme. It curiously is mentioned in their patent.

Though we were quite open in what we did and presented the information as we collected it at various symposia, some others simply wished to do the experiments in secret. I find it very difficult to think in terms of ethics of our contemporaries. I wish it were not the case. It is noteworthy that almost the identical experiment reported by Berg and associates was reported by Gouligan, Sinsheimer, and Kornberg, but used ~~in~~ DNA instead of a restriction enzyme. No one bothers to quote them. Some reputations seem to have been made solely by criticisms of this area. I do not know Cline's work at all but would have thought that the areas indicated by you should have been carefully checked to the degree possible before undertaking the experiment.

When you hear all of the difficulties we went through you will find it almost laughable.

I hope to be in Bethesda in a few weeks and hope to be able to see you then.

Sincerely yours,

beforehand

MSRH 16

Stanfield Rogers, M.D.
Professor

SER:gwb

Enclosures I hope that what we had as safety precautions will not be required in future experiments of this type. We had for example a study involving over FIFTY humans having contact with the virus - which were followed over 30 years, how it is at least 6 over 30 to 50 years, and the results are that a low blood arginine.